

normal, and again in others the profile may remain straight in biting (as in Fig. 2). As far as I could observe in Tarantola, the upper jaw was always raised in opening the mouth, and the profile of the head straightened from its normal curve when at rest, but on closing the mouth in biting the movement of the upper jaw depended upon the relative position of the animal to the object which it was biting. This depression of the upper jaw may be also often witnessed when the mouth is closed, and it may be produced by applying slight pressure to the head. The animal seems to make the most of its powers of expression, for on provocation it opens its relatively huge mouth with the greatest readiness, and will keep it open for a considerable time, during which its appearance is sufficiently awe-inspiring. The fact that the anterior part of the skull is not co-ossified with the posterior part is well known. Thus in Huxley's "Anatomy of Vertebrata" (1871, p. 225) the following statement is made concerning the geckos: "Neither the upper nor the lower temporal arcades are ossified, the post-frontal being connected with the squamosal and the maxilla with the quadrate by ligament;" and Mr. Boulenger informs me that had he been asked whether the upper jaw of such lizards is moved in biting, he would have been inclined to answer in the affirmative, reasoning from the well-known condition of the skull. But I believe it has not been hitherto actually observed that such movable articulations possess a functional value in the living animal, and that the geckos must be added to the well-known instance of the parrots as Vertebrata which move the upper jaw in biting. It is extremely probable that the same observations will be found to hold for other families of lizards. EDWARD B. POULTON

Wykeham House, Oxford, March 1

Weight and Mass

TILL some quite new facts are discovered, such as shall force us to reconsider our convictions (which have not been lightly formed), I do not think it profitable to accept a quasi-metaphysical challenge from my friend Prof. Greenhill. He has at heart, as strongly as I have, the cause of *definiteness and accuracy*:—and if he, as is natural for one in his position, feels inclined to sympathise with the "vernacular" of Engineers rather than object to it as I do, there is nothing for it but to agree to differ. My remarks on this aspect of the subject were of the most cursory and general character; and I went so far as to say that, as the book in question was written by a practical man for practical men, "perhaps we ought not to complain."

I cannot, however, go *so much further* as to allow, with Prof. Greenhill, that it is "perfectly correct" to use the words pound or ton "side by side in two senses." As regards this practice I, in turn, must quote from an unpublished letter of Clerk-Maxwell's. [The passage purports to be part of a (reported) speech by a well-known Evolutionist.]

"He regretted that so many . . . were in the habit of employing the word in a sense *too definite and limited* to be of any use in a complete theory. . . . He had himself always been careful to preserve that largeness of meaning which was too often lost sight of in elementary works. This was best done by using the word *sometimes in one sense and sometimes in another*; and in this way he trusted he had made the word occupy a sufficiently large field of thought."

I have three other remarks to make upon Prof. Greenhill's letter:—

(1) He shows the absurdity of defining the weight of a body as "the force with which it is attracted by the earth." Of course such a definition must necessarily be absurd provided it comes *after* an explanation (given by Prof. Greenhill) that "weight" is to be understood in the sense of "mass." But from this explanation itself it would unfortunately follow that *a body has weight even when it is no longer heavy*; as, for instance, when it is in a (supposed) cavity at the centre of the earth! Prof. Greenhill says that "weight" is "used in ordinary language in most cases" in the same sense as "mass." Surely the great majority of men regard weight from the point of view of the sublime Porthos:—

"Ma vaisselle d'argent . . . qui doit peser de mille à douze cents livres, CAR je pouvais à grande peine soulever le coffre qui la renferme, et ne faisais que six fois le tour de ma chambre en le portant."

(2) He also speaks of certain difficulties imposed by the "rules of language." I do not ascribe to them so lofty an origin. They are the offspring of the dogmatic ignorance which

has peopled the realms of science with Centrifugal Force and its fellow monsters.

(3) He has commented solely on a passing remark in my article, and says nothing as to its main purpose. I hope, however, that he will eschew "statical" measures of force, and give his hearty aid as well as his good wishes in the war of extermination which must perpetually be waged against the too luxuriant undergrowth of the scientific garden:—the circle-squarers, the perpetual-motionists, and (in the case before us) *the measurers of potential energy in terms of horse-power.*

P. G. T.

An Error in Maxwell's "Electricity and Magnetism"

IT may be allowed to me to remark that the error mentioned on pp. 172 and 223 of NATURE has its origin really in Helmholtz's renowned paper ("Ueber die Erhaltung der Kraft," 1847, p. 67), and that it thence found its way into most of the text-books on electricity. It has sometimes been detected and hinted at; for the first time, I believe, in C. Neumann's paper published in the *Ber. d. k. sächs. Ges. d. Wiss.* at Leipzig (1871), "Elektrodynamische Untersuchungen mit besonderer Rücksicht auf das Princip der Energie." There we find (p. 436) the formula—

$$T\nabla + T_1\nabla_1 = T^2w + T_1^2w_1 - TT_1\frac{dV_{01}}{dt} + \frac{dF}{dt},$$

which is identical with the equation of energy given in NATURE, p. 223, if we put—

$$\nabla_1 = A_1, w_1 = R_1, -TT_1V_{01} = T_m, F = T_e, \&c.$$

The formula is followed by the remark that it agrees entirely with the formula given by Helmholtz, the only difference being the last term $\frac{dF}{dt}$, which in the latter is wanting; thus we may

say that by Helmholtz the potential energy of the system invariably is expressed by zero.

Being formerly unaware of Neumann's researches, I, by another way, came to the same results, see the paper "Das Princip der Energie in seiner Anwendung auf die ponderomotorischen und elektromotorischen Wirkungen des elektrischen Stromes," published in the *Sitzber. d. k. böhm. Ges. d. Wiss.* (vide NATURE, vol. xxxii, p. 308). In this paper I have hinted at one probable cause of this and similar mistakes and their relatively difficult discovery, of which the repetition of the error in the *best text-books* is a decisive proof. This cause I believe to be the trivial circumstance that there is no consistent and generally accepted notation of the different forms of potential and energy. This renders the comparison of different writings on this subject sometimes quite perplexing. When, for example, two authors denote the *same thing*, the one by V , the other by $-V$, and when the first writes the expression $d(T_1T_2V)$ in the form—

$$T_1T_2dV + T_2VdT_1 + T_1VdT_2,$$

the other the *identical* expression $-d(T_1T_2V)$ in the form—

$$T_1T_2dV - T_2VdT_1 - T_1VdT_2,$$

it may happen that they themselves and also other readers overlook the difference of sign, and that they continue to reason as if their V were identical. Such mistakes occur oftener than is supposed. See the interesting note at the end of Sir William Thomson's paper on "Capillary Attraction," NATURE, vol. xxiv, p. 369.

Could there not be found a means of avoiding the inconveniences caused by such mistakes and the loss of labour spent in detecting them? I dare not hint at the possibility of an international system of notations of the most important physical quantities; thoroughly consistent, and recommended by the highest scientific authorities; for the realisation of such a system would probably meet difficulties quite insurmountable.

Prague University, March 12

A. SEYDLER

Tabasheer

MR. DYER in his article on "Tabasheer" in NATURE of February 24 (p. 396), throws out the suggestion that the silica deposited in the joints of bamboo may have undergone a process of dialysis. It may be of some interest to him, and to your readers generally, to learn that plates of transparent compact silica, SiO_2 , may be formed by dialysing the basic soda silicate. Four or

five years ago I discovered this, and succeeded in producing plates a quarter of an inch in thickness and four inches in diameter, by placing the basic silicate of soda within a dialyser, which was floated on dilute sulphuric acid, 1 part to 20. The plate of silica was formed in the floated vessel. A similar result may be obtained by placing in a wide test-tube a portion of basic silicate. Taking care that the upper portion of the tube is quite free from adhering silicate, the dilute acid should be poured on to the surface of the silicate without disturbing it. After a few hours the silica is eliminated in a crystalloid form.

Possibly the first process may help us to understand how tabasheer may have been deposited, while the second may throw some light on the formation of raphides, carbonic or some other acid being the active agent.

16 Savile Street, Hull, March 15

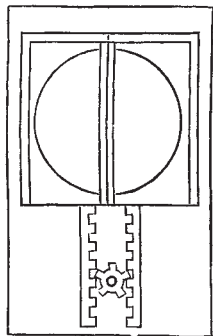
THOMAS ROWNEY

A Method of Illustrating Combinations of Colours

IN NATURE, vol. iv. p. 346, there is a description by Mr. Allen, of Sheffield, of some methods of showing the combination of various colours on a screen. He used a biunial or "dissolving view" apparatus to produce overlapping disks of colours, and also three lenses mounted close together in place of the ordinary single lantern objective, and giving images of three apertures in a lantern slide, close to which were placed cells of coloured liquids.

His experiments suggested to me the following method, which I have used for some years past, and for which only the ordinary single optical lantern is required.

A lens 10 cm. in diameter and 15 cm. focus is cut in half, and the two halves are mounted in frames so as to be capable of sliding past each other precisely in the same way as the divided object-glass of a heliometer. The motion is given by a pinion acting on racks in the same way as in the ordinary double-barrelled air-pump.



In the frames which carry the semi-lenses are cut grooves in which slips of coloured glass, or gelatine, or cells of coloured liquids may be placed; and the whole is fitted on the nose of the lantern in place of the usual objective, a diaphragm with round aperture about 3 cm. in diameter being put into the slide-holder.

Thus, when the two semi-lenses are so placed as to have their principal axes coincident, they act as a single lens and form one image of the aperture on the screen: but when they are moved past each other by turning the pinion, two disks of light are shown which can be separated entirely or made to overlap to any required extent. If, then, glasses or liquids of any given colours are placed in front of the semi-lenses, the compound colour produced by their union can be easily shown, either simultaneously with the component tints, or alone by accurately superposing the disks, thus avoiding any disturbing effect of the intrusion of other colours upon the eye.

It is in this way easy to show, taking four prominent colours, blue, green, yellow, and red, that blue + yellow = white; blue + red = purple; green + red = yellow, &c.

In place of coloured liquids, which are "messy" and liable to change, I almost always use coloured glasses, either singly or superposed (cobalt-blue, for instance, cemented to "signal-green" glass gives a good pure blue). Such glasses can by patient and careful selection be obtained of almost any required tint and intensity.

H. G. MADAN

Eton College, March 26

Ice-Period on the Altai Range

It is generally assumed that in the Altai Range there are no traces of so-called ice-ages. Hitherto, however, only ridges on the borders of the Altai Mountains have been examined. The geological phenomena of the mid-Altai regions are still almost quite unknown. In the course of last summer it happened to me to visit some parts of the south Altaic regions—the Narim Range in the vicinity of Altaiskaia, Stornitza, or Koton Karagay, the neighbourhood of the Cossack settlement Oorool, then the so-called Katoon's Pales with their snowy giant Beloocha. Subsequently I travelled in the valleys of the Belaia and Chernia Berels, and visited the valley of the Arassan lakes and some other places. Everywhere I was struck by many and various traces and remnants of a large icy cover, which has left either strong glacier deposits, or abundant remains of moraines, or pieces of granite covered with lines. The valleys, too, bear on them the indubitable signs of glacial origin. In a word, there can be no doubt as to the existence of a large ancient icy cover here. Were these glaciers contemporaneous with the ice-age of Europe and North America, or do they present themselves as a quite independent system? My own observations convince me of their independence. The Altaic ice-period had, I think, its own causes. The Altaic system of mountains is of great antiquity; and its ridges were probably much higher at one time than they are now. Perhaps the whole system rose far above the line of eternal snow, although at present this line is reached only by some of the highest summits. It is probable, too, that in those very remote times the meteorological conditions of the country were far harsher than at present, because glaciers were more numerous and descended lower, digging the V-shaped valleys of the Katoon River, of the White and Black Berels Rivers, of Chindagatooy, &c.

The question of the periodicity of glacier-ages has again been raised lately, and perhaps it is from the Altai and from the Blue Alps that we may obtain the solid data for the complete solution of this very important question.

A. BIALOVESKI

Oostkamenogorsk, November 1, 1886

A Claim of Priority

J'ai eu récemment l'occasion de lire dans le *Philosophical Magazine* (Août 1886) la description très-intéressante d'un "intégrateur sphérique," combiné par Mr. Frederick John Smith, et qui semble être une modification de celui du Prof. Hele-Shaw. Mais l'idée première de ces appareils, et c'est sur quoi je dois appeler votre attention, m'appartient sans doute, car dans le No. 630 du journal anglais NATURE (Novembre 24, 1881) j'ai donné la description d'un "Anémomètre Intégrateur," fondé sur le même principe, et qui a été plus tard cité dans le *Quarterly Journal of the Royal Meteorological Society* (No. 43, 1882), par Mr. Laughton ("Historical Sketch of Anemometry and Anemometers").

La modification imaginée par Mr. F. J. Smith, tendant à supprimer ou à amoindrir, autant que possible, le moment d'inertie de la sphère, me paraît excellente, surtout s'il fallait transmettre des vitesses quelque peu considérables. Mais quand il s'agit simplement d'enregistrer celle du vent sur une échelle modérée, je crois que la forme primitive suffit, et, d'après plusieurs essais que j'ai faits, une bille d'ivoire roulant sur des cylindres de bronze c'est ce qui donne les meilleurs résultats.

Je vous prie, Monsieur le Directeur, de vouloir bien faire constater dans votre estimable journal cette réclamation de priorité, ainsi que d'agréer mes plus sincères remerciements et l'assurance de ma considération très-distinguée.

Observatoire de Madrid, le 12 Mars

V. VENTOSA

Oktibehite or Awaruite?

IN the notice of the proceedings of the Geological Society of London (NATURE, December 23, 1886, p. 190) the discovery in New Zealand of a nickel alloy allied to *oktibehite* appears to be claimed by Prof. Ulrich, of Dunedin. This requires explanation, as the mineral was first determined, and named *awaruite*, after the locality, by Mr. W. Skey, Analyst to the N.Z. Geological Survey Department on September 28, 1885, and described by him in a paper read on October 25, 1885, and published in the local papers at the time, as well as afterwards in the Transactions of the N.Z. Institute, vol. xviii., issued May 1886. A notice of it is also given in my twenty-first annual Museum